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NATURAL LAWS AND CONTRARY-TO-FACT CONDITIONALS

By WILLIAM KNEALE

IN ANALYSIS 10.3 (January 1950) Mr. Pears argues against the view that the possibility of deriving contrary-to-fact conditionals from statements of natural law shows the latter to be something different from universal material implications of the form :

$$(x) . \phi(x) \supset \psi(x).$$

It is true, he says, that a singular contrary-to-fact conditional cannot be deduced from a universal material implication, but that is only because the former implies that its antecedent is false, whereas the latter says nothing either way about that antecedent considered in itself : on the contrary, "the impossibility of deducing its purely hypothetical element (interpreted truth-functionally) from its parent general hypothetical . . . is in fact an illusion . . . produced by concealing the universal reference of the parent general hypothetical". In support of this assertion he cites a note by Professor Popper in *Mind* LVIII (January 1949).

If contrary-to-fact conditionals were of the form :

$$\sim \phi(a) . \phi(a) \supset \psi(a),$$

what Mr. Pears says would obviously be correct. But one of the concerns of those who have recently raised the problem of contrary-to-fact conditionals is precisely to deny the sufficiency of the analysis which Mr. Pears seems to accept in his parenthetical remark . And in order to see the queerness of that analysis we need only reflect that the formula given above follows from ' $\sim \phi(a)$ ' as sole premiss. Surely a man who says "If this bird were a raven, it would be black" thinks, and thinks rightly, that he is saying more than "This bird is not a raven". If we want to use the notion of material implication in explaining what he means, we must suppose that his remark contains an implicit reference to a *universal* material implication. That is to say, we must offer some such analysis as :

$$\sim \phi(a) . (x) . \phi(x) \supset \psi(x).$$

If this interpretation were correct, a contrary-to-fact conditional would indeed follow from the negation of its antecedent taken together with a universal material implication of the appropriate kind ; for it would be just a conjunction of those two premisses. But the second interpretation seems to me no better than the

first. I do not want to dwell here on the falsity of the suggestion that a precisely formulated universal material implication can be derived from every contrary-to-fact conditional; for this difficulty might perhaps be removed by putting the reference in a vaguer form. My main reason for dissatisfaction is that I cannot see the relevance of any material implication to the proposition we are trying to analyse. In order to explain my point I shall consider Professor Popper's account of the matter.

In the note mentioned by Mr. Pears Professor Popper says that the difficulties of persons like myself arise from failure to notice the difference between terms which can be defined extensionally and those which cannot be so defined. It is true, he says, that "All my friends speak French" does not entail "If Confucius were one of my friends he would speak French", but that is because anyone who utters the first statement is thinking of the class of his friends as closed, whereas anyone who utters the second statement is thinking of the class of his friends as open. In other words, the expression 'my friends' is not used in quite the same way in the two sentences. When, however, it is said that sentences which purport to state natural laws are equivalent to universal material implications, it is to be understood that the terms involved are not mere substitutes for lists of proper names but unrestricted descriptions. And once this distinction has been made clear, there cannot, he maintains, be any serious objection to saying that statements of natural law are equivalent to universal material implications. For a universal material implication in which the terms are unrestricted descriptions does indeed allow inference to contrary-to-fact conditionals.

In this argument Professor Popper seems to assume, like Mr. Pears, that a contrary-to-fact conditional is of the form:

$$\sim \phi(a). \phi(a) \supset \psi(a).$$

But I am not concerned now with that difficulty. My present purpose is to point out that universal material implications have no relevance to contrary-to-fact conditionals. In order to make this clear it is important to consider a suitable example phrased in ordinary speech.

When we see the formula:

$$(x). \text{raven } (x) \supset \text{black } (x),$$

we commonly read it to ourselves as though it were an abbreviation of the statement:

Anything, if it is raven, is black.

Now the second expression has indeed the logical form of a suggestion of law; for the word 'if' and the timeless present are well-known devices for the formulation of laws.

But to get an accurate rendering of the original formula into ordinary English we should first transform it into the equivalent formula :

$\sim (\exists x) . \text{raven}(x) . \sim \text{black}(x),$

and then read this as an abbreviation for :

There has never been a raven that was not black,
and there will never be a raven that is not black.

This rendering, with its explicit distinction of times, has the merit of not containing any expressions which beg the question at issue, and I think it will be agreed that it does not look very like a statement of law. But let us examine it in more detail.

Clearly the second part, which refers to the future, has no special interest for a man who wonders whether in certain conditions which did not in fact obtain there would have been ravens that were not black. Is the first part any more useful to him? Surely not. For there is no incompatibility between this and the suggestion that if ravens had been tempted to live in a very snowy region they would have produced descendants that were white although still recognizably ravens. The fact, if it is a fact, that no ravens have lived in very snowy regions may be only an accident of history, and so too the fact, if it is a fact, that there has never been a raven that was not black. But to say this is just to say that, even if (*per impossibile*) we could know the second fact, we should still not be entitled to assert such a contrary-to-fact conditional as "If some inhabitants of snowy regions were ravens, they would be black".

Philosophers who treat suggestions of law as universal material implications say in effect that there is no sense in talking of historical accidents on the cosmic scale. According to their account of the matter there are only two possibilities to be considered : either (i) it is a law of nature that all ravens are black, or (ii) there has been or will be somewhere at some time a raven that was not or is not black. For if they are right, the first of these is just the contradictory of the second, and any one who tries to deny both at once abandons the principle of the excluded middle. Perhaps Professor Popper has this in mind when he says at the end of his note : "It may be useful and, for some purposes, necessary to assume a general principle stating that every kind of event that is compatible with the accepted natural laws does in fact occur in some (finite) space-time region". But if so, he does not go far enough. The principle to which he refers cannot be for him a mere assumption which we may find it useful or necessary to adopt for certain purposes. It is a direct consequence of his thesis that statements of law are universal

material implications; and if it is implausible, so too is that thesis. I shall offer two examples from different fields to show that the principle is inconsistent with our ordinary view of natural laws.

It is at least conceivable that there has never been a chain reaction of plutonium within a strong steel shell containing heavy hydrogen, and it is also conceivable that there never will be. But in order to accept these two suggestions we need not suppose that there is a law of nature excluding such events. The fact, if it is a fact, that none has occurred in the past may be explained satisfactorily by the extreme improbability of the occurrence of suitable conditions without human planning; and the fact, if it is fact, that none will occur in the future may be explained by the prevalence of a belief that such an event would have disastrous consequences.

Again, let us suppose that a musician composes an intricate tune in his imagination while he is lying on his death bed too feeble to speak or write, and that he says to himself in his last moments "No human being has ever heard or will ever hear this tune", meaning by 'this tune' a certain complex pattern of sounds which could be described in general terms. Obviously he does not think of his remark as a suggestion of natural law. It is, of course, irrelevant to my argument whether any musician has ever made or will ever make such a remark correctly. For my purpose it is sufficient that we can conceive an unrestrictedly universal material implication without regarding it as a statement of law. In short, we do not ordinarily believe that every natural possibility must be realized somewhere at some time.

What I have said about universal material implications of the kind Professor Popper considers has an obvious bearing on the problem of confirmation. Let us return once more to the formula:

$$(\text{x}). \text{raven}(\text{x}) \supset \text{black}(\text{x}).$$

Clearly this is equivalent to a conjunction of the three sentences:

- (1) There has never been an observed raven that was not black.
- (2) There has never been an unobserved raven that was not black.
- (3) There will never be a raven that is not black.

For to obtain these three sentences we need only subdivide the first of the two assertions discussed in an earlier paragraph. Now statement (1) above is very well confirmed by the evidence at our disposal. And so this evidence may perhaps be said to provide confirmation for the conjunction of (1), (2) and (3),

but only in the sense in which the fact that it is raining provides some confirmation for the conjunctive statement that it is raining and the moon is made of green cheese. When we wish to make inferences to the unobserved, (2) and (3) are the only parts of our triple assertion that can be of any use to us as premisses. But neither of these is more reliable merely because it belongs to a conjunction which has been confirmed in the Pickwickian sense just mentioned. We do, of course, think that suggestions of law may be confirmed in a useful way by observed facts, but that is only because our notion of confirmation is bound up with a policy of trying to find laws which are not merely universal material implications.

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INDUCTION AS A SEMANTIC PROBLEM

By ERNEST H. HUTTEN

THE *syntactic* problem of induction is that of the construction of a formal calculus (including rules of inference) and is usually given in terms of some concept of probability. The *semantic* problem of induction is that of trying to make clear what "induction" means.

It is not obvious what an inductive inference is. Indeed, the sense of the phrase "inductive inference" has often been questioned: Is it an inference at all? or In what ways does inductive inference differ from deductive inference? And, again, is it true as is often said that "we induce from facts" or that "induction consists in generalising from particular instances"?

To discover what may be meant by "induction", and what task induction performs, we must turn to science. Only by giving a logical re-construction of a scientific theory can the concept of induction be made clear. It is only after analysing the kinds of inference scientists actually use to establish their theories, that we can formulate rules of induction.

There are two features of scientific method which show in present-day theory. (1) Scientific method consists in constructing hypothetical-deductive systems: this has been widely recognised from the development of the more abstract theories of modern physics. (2) A scientific theory may be regarded, ideally, as a semantic language-system—that is, as consisting of a formal calculus and of an interpretation in terms of experience. This feature of theory construction was first emphasised by Einstein's distinction between physical and mathematical geometry and is now generally accepted, but its implications are not always understood. One reason for this failure is the lack until recently of a terminology of semantics; and even to-day Carnap's formulation of semantics has been applied only in a very limited field. However, it now seems desirable to make use of descriptive semantics for the re-construction of a scientific theory, because a hypothetical-deductive system can be described more precisely in this way.

In such a system the semantic rules interpret a formal calculus, that is to say, a known meta-language is used to interpret an object-language. The rules establish the meaning of a sentence in the object-language by stating the conditions under which it is said to be true; and by experience we determine whether or not these conditions are fulfilled. Thus by introducing semantic

rules the language of science is formalised and the conception of a theory as a system made more precise. If an explicit formulation of the rules cannot be given, at least a model must be provided which shows these rules; and this is a typical procedure in physics. A minimum requirement for the adequate *explication* of "induction" is that the sentences to be established belong to a definite universe of discourse and possess a logical range. It is only with respect to such a system that inductive rules can be formulated and applied. By showing the semantic method at work in theory construction, we make clear what "induction" means.

I. Inductive inference—as all inference—is determined by relations between two sentences. We do not "induce from facts". Facts do not speak: they suggest possible hypotheses but do not provide a unique, linguistic description. One and the same experiment can be used to support several hypotheses. The Bacon-Mill view that we "read off" our theories from the facts is incorrect: nature is not a book; but science is. Nor can it be maintained that we "generalise from particular instances", *i.e.* arrive by a logical process of abstraction at a universal sentence. The canons give a practical procedure for isolating causal sequences; and induction is not a search for causes but a theoretical method of confirming sentences.

II. Granted that we start with hypotheses (which are not always universal sentences), how do we come to accept them? The universal sentence allows us to deduce one or more existential sentences, by means of initial and boundary conditions. We test the hypothesis by finding out whether or not the existential sentence is true, and by this procedure the hypothesis is confirmed. But does this not make induction the same as deduction? No, for what is confirmed is the universal sentence, not the derived sentence. In deduction the logical range of one sentence is wholly contained in that of the other (*i.e.* one sentence logically implies another), and the inference is complete or demonstrative. In induction the hypothesis is only partially contained in the existential sentence (the evidence), and inference is thus incomplete or non-demonstrative. The ranges of the two sentences overlap but do not coincide, nor is one contained in the other.

Inductive inference as a relation between two sentences is analogous to implication but is not a syllogism. Only with a Principle of Uniformity is a universal sentence available as major premise to simulate a syllogism, and so induction is reduced to deduction. This rationalistic scheme of induction is generally rejected since the Principle must be a synthetic a priori sentence.

What is meant by "conclusion" in inductive inference is therefore not the hypothesis—which is an invention and cannot be logically inferred from anything—but the sentence stating the probability of the hypothesis on the given evidence. And this sentence conveys exactly the information we wish to obtain from induction.

III. How then do we arrive at a confirmation of a hypothesis? The law is confirmed by the overlapping ranges of the sentences that is, by the degree of confirmation, which is the interpretation of the concept of probability as applied to hypotheses. Can a calculus be set up for the degree of confirmation, so that we can have a logic of induction? Yes, to deny this possibility is too radical. But there may be many possible systems of inductive logic, and our demands must not be unreasonable. Both the hypothesis and the existential sentence used to confirm it belong to a semantic language-system; and confirmation is relative to the evidence which can be expressed in the system. If numerical values are assigned, they will depend upon the rules of range adopted for the system. What number is correlated to the width of the ranges of the two sentences is arbitrary but need not be subjective: just as any scale is arbitrary but can be used to determine an objective measure, relative to the scale chosen. There may be various scales depending upon extra-logical considerations—that is, on scientific practice. We do not derive or prove the hypothesis but *judge* it; and judging is always relative to a given standard. Only actual science can provide a standard for accepting a hypothesis.

The discovery of new knowledge cannot be prescribed by *rules*, although practice may suggest *procedures* for selecting the evidence. It is important to prepare our data: the best hypothesis is useless unless we "marshall our facts". In the practice of inquiry the scientist not only invents hypotheses but must also *find* the evidence. He starts with a vague hypothesis which suffices to "work out" the data at least a little; thus the hypothesis becomes clearer, and he "works over" the data once more, and so on. Hypotheses are not found in this way; but the canons are procedures of experimentation which may help us to improve them. This psychological process cannot be formalised by rules. Inductive rules show, however, whether a hypothesis is *warranted* by the evidence. To mistake a psychological process for a logical method was pointed out by Einstein to be the fault of the epistemological conception of induction.¹

IV. The difference between the scientific and the traditional

¹ Einstein, The Spencer Lecture, Oxford, 1933.

view is then this. According to epistemology inductive reasoning leads from limited evidence to a hypothesis by the rule of induction. The *predictive power* of the inference lies in the *rule* which must therefore say something about the future course of events. And since nature does not always follow our prescriptions, we say that the hypothesis so predicted is not certain, only probable. In the empiricist version the rule is expressed by saying that the relative frequency of characteristics observed in the past will persist in the future. This makes the rule itself a sentence arrived at by induction, and so we have an infinite regress and the traditional puzzle.

The scientific conception explicates "induction" within a logical re-construction of scientific theory. The *predictive power* lies in the *hypothesis*, and to invent hypotheses transcending past experience is a psychological task. Inductive rules are a means of judging the hypothesis on the basis of evidence, and relative to a language-system (theory) in which both are formulated. The inference establishes the probability of the hypothesis by rules which are formal and do not prescribe the future. Our expectation of the future is expressed in the hypothesis, and the rules *check* it against the evidence.

It is an epistemological assumption that a non-demonstrative inference must necessarily be carried out by a non-tautological rule which, I suppose, means that the rule is used as a synthetic sentence. Because the rule of enumeration is not a tautology, therefore it leads to new knowledge. But the future can never be prescribed, only conjectured, and so our anticipation of it must be given as a hypothesis. It is impossible to take the rule of induction as defining "prediction", as Reichenbach does in order to mitigate the prescriptive character of the rule. Indeed, we must say what we mean by "prediction", or we must make a hypothesis. But we cannot define what the future is to be like by saying that the definition of "prediction" . . . "will turn out to entail the postulates of the existence of certain series having a limit of the frequency".¹ We cannot always presuppose an affirmative answer to the question: Will the future be like the past? The aim of induction is not to *discover* causal sequences but to *justify* our hypotheses about them.

This disposes also of the philosophic riddle: Is induction rational? Induction as a psychological process is not rational in the sense of "according to a logical system", although it may be reasonable. Induction as a logical method is rational; but (to counter an objection) it is not rationalistic since the rules do not

¹ Reichenbach, *Experience and Prediction*. Chicago, 1938.

imply ontological assumptions. This is exactly the fault of the epistemological theories of induction (including that of empiricism) which try to make psychology do the job of logic. Neither Hume's scepticism nor Mill's belief in the uniformity of nature are solutions of the inductive problem, but rather they are attempts to banish a riddle.

Thus induction is not deduction, although deductive steps are used; it is neither an *illegitimate* inference from some to all; nor is it the *exact* inverse of deduction. We cannot infer a hypothesis by simple enumeration, not even by an elimination, of causes: variation and agreement select conditions, *i.e.* causes, not hypotheses. To say that certain events are necessary and sufficient conditions, we must know for what hypothesis they are to be conditions. Evidence eliminates rival hypotheses only if these are already known. In contrast to Broad's view we do "brood over our data": we cannot forget that what we call "data" are data for a simple, usually not clearly formulated, hypothesis.¹ There are no *absolute* data—only data for some hypothesis; otherwise, there is mere awareness, which is not a datum in this sense. The good scientist "cooks" his data since he designs an experiment in order to test a hypothesis; and its confirmation is *judged* by the experimental evidence. This is the main contribution of recent theories of confirmation.²

V. What kind of probability is the degree of confirmation? Probability is a vague concept; and many interpretations may be given of our usage of the term. Two are clearly distinguishable: the probability of hypotheses (inductive probability or degree of confirmation) and the probability of events (relative frequency). They have a mathematical calculus in common to some extent, but differ in interpretation. The fact that most (but obviously not all) of the rules of the two interpreted calculi are similar persuades us to use the same term. It is not true that inductive probability is a semantic version of frequency probability: this rests upon a misunderstanding of semantics. Frequency probability is a function whose variables refer to classes of physical events; inductive probability is a function whose argument admits sentences only. To say (as Reichenbach and von Wright seem to do) that we may replace the physical event by its name in order to pass from one concept to the other, is not compatible with logic.³ The two functions are of different logical type, that is, they are entirely different functions, since the individuals

¹ Broad, *Mind* 53, 1944.

² Carnap, *Phil. of Sci.*, vol. 12, 1945. Hempel, *Mind* 54, 1945.

³ Reichenbach, *Journ. Unified Sci.*, vol. 8. von Wright, *The logical problem of induction*, 1941

which satisfy them are of different logical type. And to change from object-language to meta-language is an interpretation, not a translation. To replace an event-frequency by the truth-frequency of the sentences describing the events is to construct *another* series of events: the sounds uttered or the marks written down which represent the individual sentences. These are not the "truths" of the sentences, for truth is not a thing or event and cannot be counted. Most hypotheses of modern science predict statistical phenomena and talk *about* relative frequencies in an infinite series of events; but a statistical hypothesis is not itself a member of an infinite series of similar hypotheses.

VI. There are many kinds of inductive inference. There is no *one* principle of induction, but various inductive rules are used by scientists for the confirmation of hypotheses. Such rules will depend on the form of the sentences involved and on the situation to which the inference applies.¹ There is no reason to assume that every sort of hypothesis is judged by all sorts of evidence in exactly the same way; and new rules may be invented with the progress of science. The traditional theory of knowledge is mainly concerned with one kind of inference only, according to which a universal sentence is confirmed by limited evidence. This is due to an incomplete analysis of the concept of law. A law may be used as an unrestrictedly universal sentence and as a general hypothesis. The difference is brought out by the instance confirmation of a law: the *next* instance of an event has the properties predicated by the law—not *all* instances (that is, a universal sentence for which the degree of confirmation must be zero). This distinction of varying degrees of universality reflects the fact that some laws, *e.g.* the Conservation laws, always function as analytic sentences.

VII. Another distinction must be noticed: we talk of the probability of hypotheses, *not* of theories. Theories are constructed for a definite universe of discourse and represent a (rather general) language-system, with primitive concepts, rules, etc. Einstein speaks of "the purely fictitious character of the basic principles of physical theory". A hypothesis is a sentence within such a system and is more or less probable according to evidence. A theory is *adequate* if it allows testable hypotheses to be formulated which suffice to describe and predict the phenomena. Fresnel's elastic theory of light is not improbable (although abandoned), but inadequate, since it breaks down in the treatment of polarization. Both a theory and a hypothesis are inventions, and there are no rules for making inventions. To

¹ Carnap, *loc. cit.*

say that a theory is probable is to assume that a theory is a sentence: but a theory is a language-system in which a sentence is formulated. So when we speak both of "Newton's law of gravitation" and of "Newton's theory of gravitation", we mean to refer, in each instance, to the inverse square relation between masses. It is impossible to assess the probability of a theory (still less of the whole of science): this is an unreasonable demand inspired by a (somewhat subdued) quest for certainty. This brings us back to the view that the probability of a hypothesis is specific to a semantic system. And the main argument is this. Only with respect to a language-system can it be decided whether a given sentence is analytic or synthetic, *i.e.* it depends upon the choice of rules according to which the sentence is used. But induction is supposed to establish synthetic sentences, and so an explication of "induction" can be given only within descriptive semantics.

VIII. The sentence expressing inductive probability is analytic with respect to a semantic language-system. But does not inductive inference establish something about the world? It does—but it is the hypothesis that is synthetic (factual), while the sentence assigning probability to it (relative to evidence) is not. That this is so has been noted sometimes with surprise. If there are rules of induction at all, it cannot be otherwise. It merely means that the confirmation has been established according to rules of inductive logic which determine probability relations between two sentences, just as the (quite different) rules of deductive logic determine other relations between two sentences (*e.g.* implication).

This is not an objection to the use of inductive probability: rather it seems to strengthen the case for distinguishing between the two concepts of probability. If a sentence of confirmation were empirical, it would itself be subject to inductive inquiry and so a hierarchy of probabilities would be generated. Again, each of these (frequency) probabilities has to be a different function since the arguments of successive probabilities only admit classes, classes of classes, and so on, unless the customary type distinction is abandoned. This hierarchy of empirical statements must be broken off by a decision which, if it is not arbitrary, represents an *estimate* of the first probability as ground for neglecting the residue, and so it must be made according to some rule.

The ambiguous usage of "probable" in ordinary language makes it difficult to recognise whether a factual assertion about a frequency (relative to a sequence of events) or an estimate of its

value (relative to some evidence) is intended in any given sentence. Relative frequency is usually regarded as a collective, physical property of events and then the estimate of its value cannot be such a property. "The probability of probability", and similar iterations needed to keep the distinction, are not grammatically acceptable and therefore they are contracted in speech. Technical terms such as "estimate" or "degree of confirmation" or "posit" must be used if the meaning of inductive sentences is to be made clearer.

IX. Self-correction is an important feature of scientific method. What do we mean by it? The analogy to Adriadne's thread in the labyrinth (given by von Wright) is suggestive but perhaps too simple. We should never get out if we were "consistently to keep to the same hand, either to the right or to the left".¹ After having kept to the same side for a while without success, we change sides, or retrace our steps. Our method is consistent but not stubborn. The picture of the maze breaks down also since there may be more than one exit in science, or none at all. The analogy suggests that only one theory (or hypothesis) is accepted and a solution to every problem found, ultimately: the history of science does not bear this out.

The mathematical method of successive approximations is the usual illustration of inductive corrigibility. In mathematics however we know the aim to be approached and are merely ignorant of the exact numerical value. An equation is assumed to possess a solution which is not directly obtainable and so is represented, say, by a power series. When the co-efficients of successive powers in the variable are determined the solution gradually improves, provided the series satisfies certain general conditions of convergence.

This property belongs to a mathematical method, but not to nature—it is not as if nature possessed ultimate characteristics which we slowly but surely discover. Nor does there exist a final world-formula, as yet undiscovered, which is gradually approached by science. Ideas of this sort are a refinement of the Principle of Uniformity and an illegitimate answer to the epistemological question: What is the nature of Nature? Self-correction cannot be conceived as a mechanism, like the governor on a steam-engine; and although a characteristic difficult to analyse it cannot be automatic. What it expresses, after all, is the simple fact that we do learn from experience, at least sometimes. We use the trial and error method; but the new trial is not a logical consequence of previous error. Whether or

¹ von Wright, *loc. cit.*

not we are inspired by failure to invent a new, and better confirmed, hypothesis is a matter of psychology. But we can judge whether we have learned from experience by applying inductive rules to the new hypothesis and establishing its confirmation.

Corrigibility belongs to inductive method in the sense that certain logical requirements must be met. A new theory (*i.e.* language-system representing a universe of discourse, or model world) is to be constructed so that it provides better testable hypotheses. A new theory is warranted only if it contains the previous one (in so far as this has been confirmed) as a lower approximation: *e.g.* quantum mechanics contains Newtonian mechanics as expressed by Bohr's Correspondence principle. And a new hypothesis is an improvement if it opens up new possibilities for testing. Popper's falsifiability criterion seems unavoidable, for a better hypothesis excludes more possible states of the world and thus says more about the actual world.¹ This shows, once more, the need for a semantic language-system in induction to specify the possible states. The corrigibility of inductive method does not consist in constructing probabilities of probabilities of probabilities, *ad inf.* (as the frequency interpretation of induction would suggest) but in making successive estimates of the same (or a different) hypothesis on the basis of different (or the same) evidence.

X. There is no problem of justification of induction in the traditional sense. Induction interpreted by a Principle, *i.e.* a synthetic sentence bridging the gap from known to unknown, leads either to apriorism or to infinite regress. It is therefore widely agreed that induction needs no justification beyond success in practice.

Justification in this new sense is formulated in Reichenbach's thesis. If success in prediction is obtainable at all, then the use of the inductive rule will lead to it, and the rule is even the *best* method for this purpose.² To avoid epistemological misinterpretation another formulation is preferable. If a successful hypothesis can be found at all, then the use of inductive rules will lead to a high probability for the hypothesis if the evidence warrants it and so select the *best* prediction *available* at the time.

The relation of hypothesis to evidence is judged according to the rules of non-demonstrative inference, and since the rules are tautologies they can lead to success if success is obtainable. For tautologies are compatible with all possible states of affairs.

¹ Popper, *Die Logik der Forschung*. Vienna, 1935.

² Reichenbach, *Wahrscheinlichkeitstheorie*. Leyden, 1935.

If the rules were not of this kind, they would say something about the world (unless they are contradictions), and this would limit their applicability. We use tautologies in order to say something about the world; but what we say about the world need not therefore be a tautology. An induction is a *better* method than haphazard guessing or the fortune-teller's tea-leaves. The fortune-teller is always wrong since he is always right, for his predictions are open to any interpretation and so are incorrigible. Scientific method is better since it is systematic, corrigible, and never final, and so takes account of all the (changing) evidence.

Indeed, the traditional problem of justifying induction does not make sense. To accept science is to justify pragmatically the use of inductive rules (or of deductive rules and of mathematics). There is no super-science which puts in doubt the whole of actual science and provides a justification for it. So there is no answer to Russell's question: "Assuming physics to be broadly speaking true, can we know it to be true . . .?"¹ It is an analytic sentence within the language of science to say that induction is the best means for establishing knowledge about the world. For any consistent and self-correcting method of establishing knowledge is called "induction".

To sum up: I have tried to point out some of the differences between the scientific and the epistemological conception of induction. There is general agreement in saying that induction consists in applying probability to sentences, but disagreement about how this is to be done. Traditional epistemology takes induction to be a psychological process and, at the same time, to follow a logical method and so prescribe the future. Only its reconstruction in logical terms can elucidate inductive method. This involves the construction of semantic language-systems (so that hypothesis and evidence can be determined as synthetic sentences) as well as the further development of systems of inductive probability. It is no argument against this thesis that an explicit and complete formulation of such systems has not yet been achieved. Actual science is not completely formalised but we can make it work all the same: just as we can use language without being able to state precise grammatical rules. It is an odd view that denies the use of semantics on this ground: like the inverse view that took deductive logic to be a finished system and thereby retarded its development for two thousand years. Nor is there any reason for saying that induction differs in ordinary life and in science. Many problems, although they

¹ Russell, *The Sidgwick Lecture*, Cambridge, 1945.

arise from the use of plain language, cannot be solved satisfactorily *in* plain language—a more precise (*i.e.* artificial) language is needed. In modern science the epistemological view of induction is abandoned in favour of the semantic approach.

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MR. STRAWSON'S ANALYSIS OF TRUTH

By JONATHAN COHEN

IN a recent paper¹ Mr. P. F. Strawson suggested that all non-technical functions of the words 'true' and 'false' could be performed, "with no very great violence to our language, . . . without the need for any expression which seems (as 'is true' seems) to make a statement" (p. 96). I wish to argue that there is at least one important non-technical function of those words which cannot be so performed. Mr. Strawson was criticising Tarski's semantic definition of truth, and I do not propose directly to attack or defend the philosophical relevance of that definition. But he also claimed (p. 83) to be elaborating something said by F. P. Ramsey in *Foundations of Mathematics*, pp. 142–143. And in this he seems to have partially misunderstood Ramsey, for Ramsey wrote explicitly that in some cases "we get statements from which we cannot in ordinary language eliminate the words 'true' and 'false'" (*op. cit.*, p. 143). Mr. Strawson's paper makes it necessary to consider this type of case at greater length than in the few sentences which Ramsey devoted to it.

Mr. Strawson's view is that when we say, for instance, "It is true that the sun is shining" we can replace 'It is true' by one of the performatory phrases 'I confirm', 'I admit', 'I concede', 'I guarantee', etc., as appropriate, without any important change of meaning. The only assertion we make, on his view, is that the sun is shining, although the grammatical form of our sentence misleadingly suggests that we are also making a second-order assertion about this first-order one. For by a "performatory" word Mr. Strawson means "a verb, the use of which, in the first person present indicative, seems to describe some activity of the speaker, but in fact *is* that activity". (He might add "or a pretence at it," for people sometimes pretend

¹ ANALYSIS, Vol. 9, No. 6.

to agree with others when they really do not). Thus, on this view, in non-technical usage the words 'true' and 'false' never function as logical predicates.

Now in respect of sentences where the statement said to be true or false is explicitly given (like "It is true that the sun is shining") this analysis is indeed an interesting elaboration of Ramsey's; and I shall make no criticism of it here. But there are other uses in which the statements said to be true or false are not explicitly given, but are merely "described", as Ramsey put it. We are told the circumstances in which they are uttered or written, but we are not given their content. Mr. Strawson appreciates the separate problem created by this type of sentence and gives three examples:—"What I am saying now is false," "All statements made in English are false," and "What the policeman said is true" (p. 92). But on his view the only relevant difference between these sentences and the others is that in uttering them we should implicitly be making "existential meta-statements" such as "I have just made (am about to make) a statement", "Some statements are made in English", and "The policeman made a statement", respectively. And he suggests that, since the words 'true' and 'false' are even in these instances not being used to make assertions at all, the truth paradoxes and their normal solutions disappear together. "The paradoxes arise on the assumption that the words 'true' and 'false' can be used to make first-order assertions. They are formally solved by the declaration that these words can be used only to make second-order assertions. Both paradoxes and solution disappear on the more radical assumption that they are not used to make assertions of any order . . . at all".

I do not think that Mr. Strawson's analysis of utterances where the statements said to be true or false are merely "described", and his consequent treatment of the truth-paradoxes (which can only arise in those cases), are satisfactory.

A judge, e.g. might, treat counsel's remark "What the policeman said is true" as expressing a statement which can be verified or falsified (shown correct or incorrect) by evidence about the policeman's character. This statement would then be taken to assert much more than merely "The policeman made a statement". The judge would be asserting a formula for indirectly verifying a number of other statements. It is not necessary to hold that the word 'true' is actually functioning as a logical predicate descriptive of the policeman's utterances. But the sentence cannot be paraphrased by "The policeman made a statement: I confirm it", as Mr. Strawson supposes,

because this does not assert a statement which could be verified or falsified by evidence about the policeman's character.

It might be objected that to treat "What the policeman said is true" as an assertion to be verified or falsified by evidence about the policeman's character is to make merely a compendious reference to the following assertions, say: "The defendant was driving at 50 miles an hour on the wrong side of the road: the plaintiff was riding a bicycle at five miles an hour on the right side of the road". And it might be urged that it is these assertions which are really being treated as (indirectly) verifiable or falsifiable by the evidence about the policeman's character. So that counsel's utterance would constitute no exception to Mr. Strawson's analysis. He merely meant: "I emphasise what the policeman said: 'the defendant was driving . . .'"

But this line of escape is not open where either the contents of the statements referred to are wholly or partially unknown, or where the number of these statements is in principle indeterminate. For if this is so the person who utters the sentence concerned cannot adequately replace it by a set of subsidiary assertions which he is emphasising or admitting. He seems to be uttering the sentence as a single, complete assertion. Thus I might say "Smith's observation-reports are always true," in a statement-making way, without having read all of his reports. Or I might say "Anything the newspapers say about Yugoslavia is false", without knowing whether any newspaper is actually saying anything about Yugoslavia; and I might be quite capable of producing evidence for my assertion.

I have been suggesting that sentences like "Any statement by this policeman is true" can be used in a statement-making way (despite their not implying the existential meta-assertion which Mr. Strawson thinks to be the only statement capable of being implied by such assertions), on the grounds that they may be treated as verifiable or falsifiable by evidence about, say, the policeman's character. It can also be argued, in support of my suggestion, that these sentences are used as statements from which inferences can be drawn: that is how the truth paradoxes occur. One of the premises from which, for instance, the judge may infer the plaintiff's liability to damages is the truth of any evidence by the policeman. And such inferences are frequently made in journalism, in historical and scientific research, as well as in the law-courts. This statement-making use of 'true' and 'false' is a very important one.

The analysis which Ramsey suggests provides a recipe for eliminating the word 'true' from such formulae at the cost of

introducing some logical jargon. But it still implies that the sentence is being used in a statement-making way. We could say, for instance, "For all p , if the policeman asserts p then p " Yet here, too, we could have paradoxes analogous to the truth paradoxes. Thus a symbolic formula for expressions which could be used to analyse sentences like "Any statements asserted by me are false" would be $(p): (x). \phi(p, x) \supset. \sim p$, where $\phi(p, x)$ is to be interpreted as "The statement that . . . is asserted by . . ." This would give rise to a paradox analogous to a truth paradox if the expression as a whole were believed capable of occurring as a value of p .

I can, however, conceive of someone's uttering the sentence "Anything the policeman said is true" as a substitute for "The policeman is a reliable witness". And if this were a straightforward description of the policeman's character in the light of the evidence about it no paradoxes would arise when I said, in the same way, "Any statements asserted by me are false". I should merely be describing my character. But I think legal counsel, for instance, would use the sentence "The policeman is a reliable witness" rather as a recommendation, which the evidence about the policeman gives ground for approving, than as a description. And if I said "My utterances are quite unreliable" in this way I should give rise to a paradox. I should be recommending people not to rely on any of my utterances and therefore to refrain from relying on the utterance "My utterances are quite unreliable". In other words I should be at the same time both recommending them not to rely, and discouraging them from not relying, on my utterances. Moreover, the solution of this paradox would be analogous to the normal solution of the truth paradoxes. I should have to specify the type of utterance which I considered unreliable. I should have to say, perhaps, "My descriptive utterances are quite unreliable", leaving it open whether my recommendations, arithmetical utterances, and so on, were reliable. So that I can use the word 'recommend' in a performatory way, in, say, "I recommend you not to rely on my utterances," and still give rise to a paradox that requires solution by a theory of types (although it may not be quite like the paradoxes of formal logic.)

Thus, to sum up, when it is correct to paraphrase sentences like "Anything the policeman said is true" by sentences like "The policeman is a reliable witness", and they are used as descriptive statements no paradoxes analogous to the truth paradoxes can arise. But if they are used as recommendations, as they commonly are, and not as statements at all, these may

arise. And when such a paraphrase cannot be made the sentences seem to be used to state a formula for the indirect verification of other statements. This formula may itself be judged 'true' or 'false' (or 'correct' or 'incorrect') and can give rise to logical paradoxes. The recommendation-analysis is more in keeping with the section of Mr. Strawson's analysis of 'true' and 'false' to which I am not objecting, where the statements are not just referred to but explicitly given; but it does not bear out his claim to be able to dispense with the normal solution of the truth paradoxes. The analysis in terms of a verification-formula is the one suggested by Ramsey, whom Mr. Strawson claims to be elaborating; but this both give rise to logical paradoxes and also implies that 'is true' is being used to make an assertion. Mr. Strawson may still be correct, however, in holding that 'true' is not used as a logical predicate, since in the description-analysis the logical predicate is 'true-statement-maker', in the recommendation-analysis there is no logical predicate, and in the verification-formula-analysis we can eliminate the word 'true' (although not in ordinary language).

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PHILOSOPHY AND THE COMMON READER

By P. H. MARRIS

WHEN a philosopher who has academic training discusses his subject with someone who has none, he is often met with puzzlement and sometimes with contempt. For envy at the courage or good fortune that has enabled him to devote himself to the most profound and important of all questions turns, when the true nature of his studies is revealed, at best to a grudging admission that philosophy may be "quite good fun for people who like that sort of thing".¹

When I mentioned to a scientist, for instance, that scientific discoveries were of no help with philosophical problems, he was much taken aback. Did not philosophers discuss such topics as the beginning of the world? They did not, I said, or at least not *e.g.* theories about the cooling of vortices of gas, for these were matters of scientific investigation with which philosophers did not concern themselves. Geologists find a flint here, a strata of rock there, and so set about arranging their findings in chronological pattern accounting for what they already know and suggesting what is yet to be discovered. But this philosophers consider to be a matter for the geologists themselves to discuss, and not being geologists do not therefore intervene.

However, though they do not practice geology, they may study the practice of geology as an instance of scientific method. And they might also be concerned with "the beginning of the world"—as a phrase between inverted commas—if its logic interested them. Satisfied with this explanation, he dismissed philosophy as a subject of very limited interest, and wondered why I, whom he knew to have been once interested in questions of a very different sort, bothered myself with it.

Again, "It is easier to know how to build bridges", said an engineer, who had interrupted his career to take a course in philosophy, "than when and where to build them". Such problems seem often to turn people towards philosophy, and are yet so different from any that professional philosophers discuss. He was already impatient with his course, for however much he learned of the possible definitions of 'good', the meaning of 'right' and 'wrong', the analysis of moral obligation, he was naturally no nearer knowing when or where to build bridges.

Thus it seems to be popularly expected of philosophy that it will yield both knowledge of matters of fact, and knowledge

¹ The phrase with which Professor Broad concludes his *Five Types of Ethical Theory*.

of what our ultimate aims should be, and also, I think, that in some way the second must follow from the first. When it is found not to be concerned with this, but rather with logical analysis and the meaning of words, there is general disappointment. It seems as if an unlucky accident had given the same name to two wholly separate lines of thought, and that many of those who now choose to study philosophy should be disabused of a misconception, and saved disillusionment.

But of course the lines of thought are not wholly separate, because one of the principal aims of logical analysis has been to unravel the confusions and obscurities of metaphysics. Professor Moore, for instance in his "Autobiography" remarks that he might never have become interested in philosophy but for his astonishment at what philosophers were saying. And those who, searching amongst the work of the metaphysicians for an answer to their questions about the nature and meaning of life were also astonished; welcomed the freshness of this new approach. But the outcome of logical analysis has been the conviction not only that metaphysics failed to answer such questions, but that they cannot sensibly be asked. For in order to make sense of the questions they must be turned into problems of the natural sciences or requests for advice, and then they are no longer the questions we wished to ask. It is in saying this that contemporary philosophers deal their most disheartening blow.

"We understand," they explain, "that when you ask 'What is the meaning of life?' you are not asking 'What is the meaning of 'life'?' " If we are to make any sense of your question at all, we must suppose you want to know the purpose, the end of life. But a factual answer, for instance that at last a dying sun will warm the earth no longer and life will be extinguished, does not satisfy you. Yours is a request for moral guidance, if we are right, a desire to know what ends are most worth pursuing, what is most highly to be valued. And this everyone must decide for himself. If he appeals for advice, only he can decide whose advice he will take. Wisdom comes only through experience: there is no short cut. You also ask "What is the nature of life?" almost as if it were the same question, but from no amount of knowledge of its nature will any moral judgements follow: for such a judgment to be contained in the conclusion of an argument, there must also have been one in the premises. The failure of metaphysics is, indeed, largely the failure to realise this".

But if it is true that philosophers have failed to achieve what was popularly hoped of them, because it is impossible they should have succeeded, how is it that those who are puzzled about the "meaning of life" should yet find something in metaphysical writing which helps them toward the understanding they desire? To this it is often replied that metaphysics may sometimes be accepted indulgently as a sort of poetry. Yet why poetry of any sort should satisfy those who ask "What is life?"—not seeking now for moral guidance, nor perhaps putting any other question, but expressing puzzlement, a desire to understand—is not explained.

When someone looks back over his life, musing, wondering what to make of it; or hums to himself "What is this thing called love?", having been in love several times, he is not now in search of information. He turns his memories over in his mind, trying to grasp the quality of his experiences, not to remind himself of the fact of them, and the more precise and comprehensible the language he chooses to describe, the more this quality eludes the description. "Born on the third of February, towards midnight. Educated at Harrow and Cambridge. Stood as Unionist candidate for Belfast . . ." "I first met her in Bermondsey. She left me in Rome". These are facts, and just because their meaningfulness is beyond question, they seem to mean so little when we pause to reflect. They disguise the mysteriousness of what they are about. And as we improve the description—adding, perhaps, that when I met her in Bermondsey her eyes shone like fine porcelain seen against the light—so it approaches poetry. When, therefore, someone after reflecting on life feels he begins to understand it, it is perhaps only through poetry that he can express his understanding, the sort of poetry that sometimes appears as metaphysics.

There is this passage in the *Tractatus Logico-Philosophicus* which seems to express what I wish to say:

"6.521. The solution of the problem of life is seen in the vanishing of this problem.

(Is not this the reason why men to whom after long doubting the sense of life became clear, could not then say wherein this sense consisted?)"

The purpose of this paper was to suggest that the most fundamental aim of metaphysical writing is to say "wherein this sense consisted". For lack of a means of adequate expression the attempt seems clumsy, paradoxical, a failure, and in the confusion the original purpose has been largely lost. Yet the failure is not always complete for all readers: some hints of what the

authors meant survive, the attempt seems better than silence, however much silence would have been more correct.

Thus it is not difficult to see why people should be uneasy at the passing away of metaphysics. For I think this desire for understanding, this sense of a mystery, will always remain to trouble them, and that it is not merely the misfortune of those who are neurotic. There is of course no reason why professional philosophers should concern themselves with this desire, but unless they respect it their criticism of metaphysics is in danger of being insensitive and superficial. A suspicion that they do not always respect it lies at the back of the common reader's impatience with contemporary philosophy. And the suspicion is not always unfounded.

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NOTES

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